At the suggestion of du Bois-Reymond the author undertook an investigation of the phenomenon. After excluding more or less completely by experiment or well known physical principles the possibility of the sensation being due to the dampness of the gas, its conductivity, its heat capacity, its absorbent power, its setting free heat in its absorption in the moisture of the tissues of the skin and its causing an elevation of the skin temperature by dilation of the small blood-vessels, he concludes that it is really due to an actual chemical stimulation by the gas of the nerves of warm sensation.

Thermische Experimente an der Küchenschabe (Periplaneta orientalis). V. GRABER. Arch. f. d. ges. Physiol. XLI, abstract by Hermann in Jahresb. Anat. u. Physiol. Bd. XVI, Abth. 2, 1888.

The limits of temperature fatal to these roaches are —6° C. and 41°. With decreasing temperature, at about 5°, they lose locomotion and, if they remain at that temperature, other power of motion also. They will still respond, however, to strong stimulation. Below 0° they soon become paralysed, but recover more or less perfectly when warmed again. At —5° or —6° they die in from 10 to 20 minutes. Increasing temperature makes them more lively; above 37° they go into convulsions, and die slowly at 41°, though for five minutes or less they can bear 60°. Graber tested the temperature preferences of these animals by an apparatus of three connecting chambers, the two outer ones of which were of variable temperature. If the side chambers were both high, say 38°, the insects all stayed in the middle one. If they differed by about 2° and were still high, most of the animals chose the cooler. If the side chambers were both cold, they picked the warmer. The roughness and conductivity of the floor were of great influence. The "optimum" or temperature of greatest preference was about 26° or 28°, but at this very point the animals were frequently uninfluenced in their choice by wide differences of temperature. When offered a very hot chamber and a very cold one, they preferred the hot one up to about 39°, or only went into the other for a little while to cool off. When the hot chamber was yet hotter, they preferred the cold, even if below zero. Strange to say, they did not in these experiments remain in the middle chamber.

Die räumliche und zeitliche Aufeinanderfolge reflectorisch contrahirter Muskeln. Dr. WARREN P. LOMBARD. Separat-Abzug aus Archiv f. Anat. u. Phys. 1885.

To know a reflex act one must know the muscular contractions that enter into it and their order and extent in space and time. Such an analysis Dr. Lombard made for the reflex contraction of the muscles of a frog's leg. He found that the reflex called out by a continuous heat-stimulation was not a continuous contraction, but one broken by periods of rest; also that the order of contraction of the muscles in a series of reflexes was not constant; that, other things being equal, the number of muscles excited, and the length of time required for the stimulus to spread to all the motor roots, varied with the kind and intensity of the stimulus. From these he concludes that there must be somewhere in the central portion of the centripetal-centrifugal arc an apparatus that holds back the

incoming excitation till it has reached a certain intensity and then transmits it to the motor roots; and that there is independent connection between the centripetal nerve and each motor root controlling muscles that enter the reflex. The difficulties involved in the older assumption that the order of contraction is fixed by the various rates of central conduction are avoided by supposing that the order depends on a difference of excitability of the structures connecting the sensory and motor roots. The grade of excitability would depend on chemical conditions, which can change quickly and in limited areas, and so produced the variable order of contraction found. The importance of chemical conditions is apparent in the strychnized frog where the differences of the periods of delay for the different muscles are abolished.

Relation de diverses experiences sur la transmission mentale, la lucidité, et autres phenomènes non explicables par les données scientifiques actuelles. CHARLES RICHET. Proceedings of the Society for Psychical Research, Part XII.

This lengthy article of 150 pages with so startling a title, coming from so prominent a scientist, is sure to attract one's attention. In a topic where so much bad method has prevailed, one expects much from a trained scientific thinker. Unfortunately this expectation is doomed to disappointment. M. Richet's application of the theory of probabilities to his results is very shallow, and the nature of his evidence often entirely too subjective. To begin with, his subjects are four hysterical women, for whose honesty we must be satisfied with Prof. Richet's declaration in their behalf. The first test consists in his willing one of his patients to go to sleep when the latter is at a house several hundred yards distant. Upon going to the house he hypnotizes the subject, who then informs him of the time during which he attempted to will her to sleep. The experiment is varied, but the time given by the subject is in Prof. Richet's opinion so often near the truth that chance fails to account for the successes. Again, hundreds of trials are made to transfer a simple drawing from Prof. Richet's mind to that of the subject. A large number of illustrations record the more successful cases, but the new fact that is emphasized is the discovery that the reproduction was almost equally successful when M. Richet himself was unaware of the character of the drawing to be transferred. This leads him to postulate a state of "lucidity" in which mental impressions are possible without the ordinary aid of the senses. Again, he experimented with a group of sixty drawings with normal subjects, and found on the average seven successful "transfers" in two hundred trials, while with his selected subjects he obtained twenty successes in the same number of trials. The subject while in the hypnotic state attempts to describe the disease of a patient, a lock of whose hair she sees; the descriptions are vague and do not impress the unprejudiced reader as at all noteworthy. Experiments in guessing cards were tried, but the number of successes was not above what chance would account for. This only sketches a small portion of this comprehensive study, which must be read in the original with account of precautions and the illustrations of results.

M. Richet enters upon his research with what appears, in the light of a sound logic, an utterly false notion, namely, that chance or a